Statistical models for Sacramento late fall run and San Joaquin fall run chinook salmon

Ken Brian Newman

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0334: Statistical models for Sacramento late fall run and San Joaquin fall run chinook salmon

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: This proposal will construct a statistical model for survival of fall run Sacramento and San Joaquin Chinook salmon, using a state−space approach and historical data sets of recovery of marked and tagged juvenile Chinook salmon. The goals of the model are to better understand factors influencing in−river survival and the relative role of water operations. The investigator is highly qualified and this type of model will likely be useful for resource managers, but the availability and quality of data required to produce reliable results has not been demonstrated, making project success uncertain. Where specifically were data collected, how was it done, what is the quality of the data, how specifically will these data be used in the proposed model, and what specific hypotheses will be tested with the available data? Alternatively, given the existing data and their limitations, will this approach produce substantially better understanding of factors influencing survival than what is already known? The project has strong potential, but would benefit from further discussion of data requirements, likelihood of practical success, and need for this approach compared to existing knowledge/models. The technical reviewer ratings were excellent, good, and good, but the first review was discarded because it lacked substance and was not very critical. Despite

the remaining “good” ratings, I found the above criticisms substantial enough to consider the project flawed, and I assigned a rating of inadequate. Goals: Project goals are clear (modeling survival of fall run chinook), but no stated hypothesis. Timely in terms of quantifying factors affecting survival of threatened/endangered salmonids and for assessing management options/tradeoffs. Justification: The project is justified in terms of the need for understanding factors influencing Chinook salmon survival, and the PI has done an excellent job framing the project within CALFED initiatives/priority topics. However, it is unclear that this approach will provide better information/understanding than is currently available to managers. Moreover, the availability and quality of data needed to implement the model and produce reliable results has not been demonstrated. One reviewer stated concerns about the author’s access to the needed data. Approach: The approach is appropriate for meeting the stated objectives, but as discussed above, it is unclear that data availability and quality are sufficient for constructing the model. For example, what is the quality of abundance measurements and how might this influence model parameters, performance, and ability to correctly identify survival factors? Reviewer 2 also raises minor questions regarding parameter formulation. It was also unclear how ocean catch rates provide any additional information to the catch rates of out−migrating juveniles in the river. Feasibility: Approach is feasible in general terms (model can be fit/generated), but uncertain how successful it will be given above concerns regarding data availability and quality. Monitoring: Not applicable. Products: Primary product would be useful for understanding factors controlling survival and for assessing management options, but depends on success of model, which is uncertain. Capabilities: The PI is highly qualified and has considerable prior experience with this type of work. Budget: The budget seems excess and several issues were raised by reviewers: 1) appropriate for the PI to bill as a consultant ?; 2) PI involvement seems low (20 days/year); and 3) PhD student salary is high (more typical of a well−paid postdoc in the US).

Additional Comments:

Summary: This proposal will construct a statistical model for survival of fall run Sacramento and San Joaquin Chinook salmon, using a state−space approach and historical data sets of recovery of marked and tagged juvenile Chinook salmon. The goals of the model are to better understand factors influencing in−river survival and the relative role of water operations. The investigator is highly qualified and this type of model will likely be useful for resource managers, but the availability and quality of data required to produce reliable results has not been demonstrated, making project success uncertain. Where specifically were data collected, how was it done, what is the quality of the data, how specifically will these data be used in the proposed model, and what specific hypotheses will be tested with the available data? Alternatively, given the existing data and their limitations, will this approach produce substantially better understanding of factors influencing survival than what is already known? The project has strong potential, but would benefit from further discussion of data requirements, likelihood of practical success, and need for this approach compared to existing knowledge/models. The technical reviewer ratings were excellent, good, and good, but the first review was discarded because it lacked substance and was not very critical. Despite the remaining "good" ratings, I found the above criticisms substantial enough to consider the project flawed, and I assigned a rating of inadequate. Goals: Project goals are clear (modeling survival of fall run chinook), but no stated hypothesis. Timely in terms of quantifying factors affecting survival of threatened/endangered salmonids and for assessing management options/tradeoffs. Justification: The project is justified in terms of the need for understanding factors influencing Chinook salmon survival, and the PI has done an excellent job framing the project within CALFED initiatives/priority topics. However, it is unclear that this approach will provide better information/understanding than is currently available to managers. Moreover, the availability and quality of data needed to implement the model and produce reliable results has not been demonstrated. One reviewer

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The panel agreed with the primary review that while the modeling approach is useful, the panel was not convinced that the availability and the quality of the data were justified for the approach, making success and value uncertain.

Rating: inadequate.

Technical Review #1

proposal title: Statistical models for Sacramento late fall run and San Joaquin fall run chinook salmon

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals of this project, namely to identify and quantify the effect of environmental conditions on the survival rate of juvenile salmon, are timely, particularly given the potential impact that water management actions can have on these imperiled stocks. The project does not have a clearly defined a priori hypothesis, but an implicit hypothesis is that survival depends on environmental covariates. The objectives are clearly identified, and achieving these objectives would provide valuable insight into the factors affecting juvenile survival, and useful guidance for potential management tradeoffs.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The author proposes to use state−of−the−art statistical modeling to perform an integrated assessment of all relevant data. The state−space modeling framework described is a powerful conceptual basis for the research, and provides an appealing approach to**

	addressing important questions regarding the factors affecting the dynamics of juvenile chinook salmon. The author does not provide convincing evidence, however, that there exists adequate information in the data available to fully take advantage of this modeling approach, nor does he provide a clear indication of the extent that this approach will provide "better" information for resource managers. Although this is a powerful modeling tool (which I in fact teach the basic elements of in my own modeling course), the author needs to better develop the justification for this as opposed to other approaches.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The general approach proposed in this project is very appropriate to the objectives listed, and is at the cutting edge of current statistical analyses of fishery systems. As such, it has the potential to provide novel information to decision makers, and will likely result in refined tools for the analysis of similar situations. My major concern with the proposed approach is the lack of detail regarding the data available to estimate the parameters in this modeling framework. As described in the proposal, the models envisioned include parameters for time-varying survival, and include fish abundance as a state variable. A considerable complication is the fact that fish movement between sites is also a time-varying parameter that is potentially completely confounded with survival if the right type of data are**

	unavailable. Without further details on the type of data available, it is impossible to judge whether the models are even estimable. While determining the estimability of the model is identified as a component of the overall study, it is critical before launching into a full-blown modeling effort. **Beyond the estimability of model parameters, it is impossible for me to assess the likelihood that the model will produce results sensitive to the influence of environmental conditions. Although my experience with such models is much more limited than Dr. Newman's, I have found that details regarding contrast in the data structure (e.g., the amount of variation in environmental characteristics, the independence of variation among environmental conditions) can have a large influence on what information these models are capable of extracting, and what parameters become hopelessly confounded.** **As a very minor point, the observation process equation presented on page 4 of the proposal is a common form used as an example of a process with lognormal errors. I would note that the expected value of y sub t increases as the variance of the normal distribution increases. This occurs because the expected value of the exponentiation is only 1.0 (as is intended) when the variance of the normal distribution is zero. For positive variance, the departure from a multiplier of 1.0 is the familiar difference between the mean (expected value), and the median. I truthfully don't know if this creates bias in the estimation procedure, but have seen papers discussing the importance of transformation bias on parameter estimation and predictions from nonlinear regressions.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**As indicated above, I feel that too few details on data availability are presented to allow me to judge whether it will be feasible to fit the intended models. As such, I feel I must rate this aspect of the project rather low.**
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Not applicable**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**If adequate data are available, this project has the potential to provide useful informational products (e.g., better understanding of water management actions on juvenile salmon survival), and has the potential to provide valuable analytical and decision−modeling tools.**
Rating	**very good**

Additional Comments

Comments	None

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The principal investigator is at the forefront of this field, and has made strong recent contributions to this area. As such, I feel that he is one of the best suited individuals for implementing a project of this nature.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	I feel that the project budget is very high, given that the entire focus of the project is on data analysis, and no new data acquisition is proposed. A consultancy fee of over $1000 per day of effort seems high to me, as does a student stipend and fee cost of $46,800 per year. The overhead rates (which are out of the PI's control) further enlarge the budget. In contrast, the typical cost to support a student at my university is approximately $26,000 with a 45% overhead rate.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	Although my overall rating on this project is relatively low, and I would not recommend funding the

	current request, I would like to emphasize the great advancement that this approach presents over "traditional" statistical approaches. When I teach this subject in class, I use the analogy with students that this can be viewed as true fishery dynamics, while other approaches are fishery "statics". I would personally value the contributions that this research would make in my area of interest, but feel that a preliminary overview of the actual data available is essential before deciding to embark on such an endeavor.
Rating	**good**

Technical Review #2

proposal title: Statistical models for Sacramento late fall run and San Joaquin fall run chinook salmon

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals, objectives and hypotheses are very clearly stated. This work is one important approach to the analysis of the salmon recovery data that should be investigated.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is justified because of the urgent need to understand the factors that influence salmon survival.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	

	With a project like this there is obviously some uncertainty about the extent that the objectives will be achieved. It may be that there are reasons why this particular modeling approach is not appropriate for the data. Nevertheless, the approach seems definitely worth trying, and should produce some information that is useful to decision makers.
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is fully documented to the extent possible at this stage. Certainly the proposed approach is feasible, and will almost certainly be successful in terms of fitting a model. The author also certainly has the skills required to do the analysis.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Not appropriate.**
Rating	**excellent**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	

	I believe that products of value will come from the project, if only because they will give another way of looking at the salmon recovery data. Uncertainties about the model assumptions may need to be taken into account when assessing the outcomes.
Rating	very good

Additional Comments

Comments	Basically I believe that this modeling approach is worth trying, even if the outcome is not as clearcut as managers would like. What I mean by this is that the final model will be quite complicated and, no doubt, there will be questions about the extent to which it represents reality and can be used for management actions.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The principal investigator has an excellent track record. I am not sure whether he will be able to find a suitable PhD student, which will be crucial to the project. The infrastructure and support at the University of St Andrews should be very good.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget concerns me. The PI has only put down 20 days work per year at a cost of about $28k with travel. This is about one quarter of the budget. In fact, I do not think that 20 days per year is at all adequate for the PI's time on this project because his

	contribution will be crucial. This means that either he will not be able to make the necessary contributions or his time will be paid for from some other source. The PhD student and overhead costs which comprise about 3/4 of the budget seem too much to me. I would have preferred to see a budget with much more time for the PI and much lower other costs.
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**This project is worth doing, and should obtain its objectives. The PI has the experience and ability to get the expected results. I am concerned that the PI is allocating only 20 days per year to the project, which seems inadequate. This is particularly the case as he will need to travel to the US once or twice a year for discussions about the work. At present the proposal relies too much on an unknown PhD student. Therefore I think that the project is worth doing but maybe there should be discussions with the PI about how the work is carried out.**
Rating	**good**

Technical Review #3

proposal title: Statistical models for Sacramento late fall run and San Joaquin fall run chinook salmon

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**This is a well−written and logically organized project. The goals are clearly stated and although the hypotheses were not explicitly described the flow and focus of the text was sufficiently clear. I am surprised that this work has not been done before.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Strong justification for the work. This builds very well on existing knowledge as the PIs will use historical data that are already in hand. These types of models have beeen successfully developed and used before and would be an important tool for understanding and managing the Chinook salmon stocks.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**This work should result in the development of a useful modeling tool for policy makers. The approach is not entirely novel but it is feasible and has a high likelihood of success.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**High likelihood of success given the track record of the PI and the fact that the data are already in hand.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Should result in a practical model for monitoring and managing salmon releases.**
Rating	

	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Excellent qualifications. PI appears to be a rising star in the field and has an impressive track record of pertinent publications and experience.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Only question the consulting fee for the PI. Is this a consulting job (>$1,000/day)? I am not accustomed to seeing university researchers receiving consulting fees for projects that they are working on under the auspices of the university (overhead charged on labor costs). Ultimately, the suitability of this arrangement is for the funding agency to decide but the consulting fees have the effect of significantly increasing the budget.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	Solid, well-designed project with high probability of delivering a practical tool for decision-makers.

Technical Review #3

Rating	excellent